Additional Comments on Proposed MFL Criteria
for the Loxahatchee River and Estuary
Submitted by: Merryl Alber Dept. of Marine Sciences, Univ. of Georgia
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This is a response to the two documents that were sent to be by the district as a result of my comments on the July 2002 draft of the Loxahatchee River MFL. The first is the draft response to my peer review comments, and the second is a Technical Memo prepared by Dr. Gordon Hu regarding the hydrodyamic model and regression analysis.

District Response to review

The District has done a thorough job responding to the points raised in my draft review of the proposed MFL criteria for the Loxahatchee River and Estuary. I am pleased with this effort: many of the points I raised have either been taken into consideration or have been explained to my satisfaction. In a few cases, however, I feel there is a need for further discussion (page numbers below refer to the District's response document)

p. 2 Model calibration – the district has now provided color copies of the graphics presented in Appendix P. It is a little curious that the Figure for station 65 is the only one that provides output for both the calibration and verification stages of the model (only calibration is shown for station 64 and only verification for 66). Given this limited amount of information it is unfortunate that the model misses one large spike in the field data, and that field data are missing during much of the verification stage. I agree that as far as it goes the model does largely capture the range of the salinity observations, but it would be useful to see additional verification when the data become available. The model also seems to have a much larger dynamic range than field observations at station 64.

p. 3 Despite the model's problems, the District has decided that it is preferable to the regression analysis for predicting salinity under different flow regimes. I am willing to agree with this, but I have several points that I would still like to see addressed. These points are taken up below in my response to the Technical Memorandum by G. Hu.

p. 4, 1st paragraph My review suggested that historic salinity data, where available, could be used to spot-check model predictions of salinity at various places and times in the past. Appendix A describes several studies that might be appropriate for this purpose. This point does not seem to have been addressed.

p. 4-p. 5 The District suggests that the basin's storage capacity has not changed, and provides information to show that the flow duration curve derived for 1971-1984 is similar to that for 1990-2001 during low flow periods. However, the analysis in Table 24 of the original document (p. 98) suggests that even if the same percentage of time is spent at low flows, there are fewer events (35 periods during 1990-2001 where flow was less

than 20 cfs versus 59 during 1971-1989) so it is not completely clear that the low flow conditions have not changed. The District might want to revisit this analysis.

Even if low flow hasn't changed, however, that does not justify using current flow/salinity relationships because the point is that closing the gaps has changed the river and thus the salinity could have changed even during the same flow. The analysis presented by Dr. Hu suggests these changes occurred downstream of RM 8, which is fortunate in terms of setting the MFL criteria. However, this point should not be ignored when evaluating predicted salinities further downstream.

On a minor point, I'm confused over why there is limited water storage capacity during dry periods. Wouldn't that be a more important consideration during wet periods?

p. 6. Whether or not there has been a measurable increase in salinity over the past decade is an important question that needs to be addressed. In my review I suggested that the District recalculate the information obtained from the Wild and Scenic Segment of the river without station 63 to determine if average salinities have in fact increased over the past decade (see p. 102). Either this or some other way to determine whether salinity has increased in response to the general increase in flow would be really useful.
This point is made at the bottom of p. 6 in the District's response, but the line on the top of p. 7 is not a response to this comment.

p. 9 and p. 13 I understand that the goal of the MFL is to try to apply the flow regime that now occurs at 10.2 to that at 9.2, and that in essence the proposed MFL was an attempt to shift the low end of the flow duration curve up by reducing the frequency of low flow events. I am concerned (as was Dr. Kent) that the District's choice of 35 cfs may not meet this goal. As we have both noted, an average flow of 100 cfs would be a better way of ensuring that salinities at RM 9.2 average 0.15, which is what they are at 10.2 Although the response explains that this was an attempt to define average conditions, it might be worth considering a statement that relates to the flow duration curve at more than one point (e.g. whenever flows are less than 35 cfs for more than 20 d (not more than once every 6 y) OR less than 47 cfs for more than 30 d (not more than once every 1.6 y) OR less than (use other percentiles…), etc. This would avoid the possibility that flows could be kept at slightly more than 35 cfs without consequence and might be more in keeping with the District's stated goal for the MFL.

p. 12 I'm still not convinced that the differences between the long-term salinity modeling effort and the flow statistics are due to "largely randomized errors" or that the potential bias in the long-term flow and salinity estimates is "less important than being able to estimate how the system would perform under a wider range of hydrologic conditions." The wide range of variation in observed salinities at a given flow suggests that the model does not account for all of the variables (besides the tidal cycle) that can potentially affect salinity. A smoothed model is perhaps more tractable, but if it is not accurate it does not make sense to use the model to predict the system performance. Although I am willing to accept that it is inappropriate to compare the flow data to the salinity observations (as I tried to do), this again comes down to the need for additional model verification (both with current data and with historic information) to reassure users that the model is appropriate.

Technical Memo

This memo is largely focused on the work that the District has done to evaluate the flow-salinity relationships derived from regression analysis and compare these results with those of the hydrodynamic model. I am pleased to see that the District has been so responsive to my comments in this regard. My comments below are both reactions to what has been done to-date and suggestions for further refinement of these comparisons.

1. I am glad to see the District is pursuing the SAS analysis. When this analysis is complete, it would be nice to see the regressions and the statistics associated with them to know if they are indeed fitting the data well. If they are not, the next layer of analysis (e.g. as done in figure 3) is not warranted.

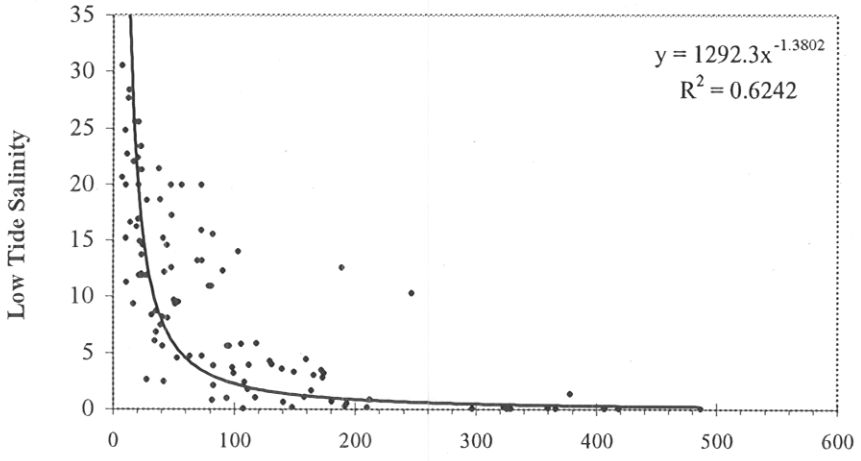
2. I agree that the SAS analysis presented for Station 64 does not provide a good fit for the high flow/low salinity data (although I might argue that it is the fit for Station 65, which is much better, that is more important in terms of the MFL). These fits could be improved by using a better time lag for data averaging.

In our experience matching each observation with the appropriate discharge can substantially improve the "tightness" of the relationship between flow and salinity. I am enclosing graphs of 2 different sites in Georgia to illustrate this point. (These are large estuaries where monthly discharge seemed appropriate as a first cut.) In both cases the salinity observations are matched with a) the discharge during the month when the observation was made, b) the discharge averaged over the month previous to the observation, and c) discharge averaged over a variable period that depended on flow. Although it is necessary to IGNORE the equations on this graph (they were fit with EXCEL before we learned to do things better), it is clear that much of the variability in these observations was due to the changes in the discharge and could be reduced by taking that into account.

Another way to improve the fits is to use only a segment of the data (as has been done here). However, if the low flow/high salinity periods are most important I'm not sure it makes sense to focus on the 0-10 ppt range of the points. What happens to the SAS fits when one confines flows to less than 100 cfs and ignores the high end of the data?

3. If I understand this properly, the "Linton" model is the one that was presented in the May 2001 draft MFL document. If so, this relationship is based on the EXCEL regressions, which we **know** are faulty (look at the curve fit for station 65 on page D-7 compared with p. D-21!). It is therefore not reassuring to see a close fit between the Linton and the hydrodynamic model.

Ogeechee River, Off Harvey's Island
LMER 14.3 km



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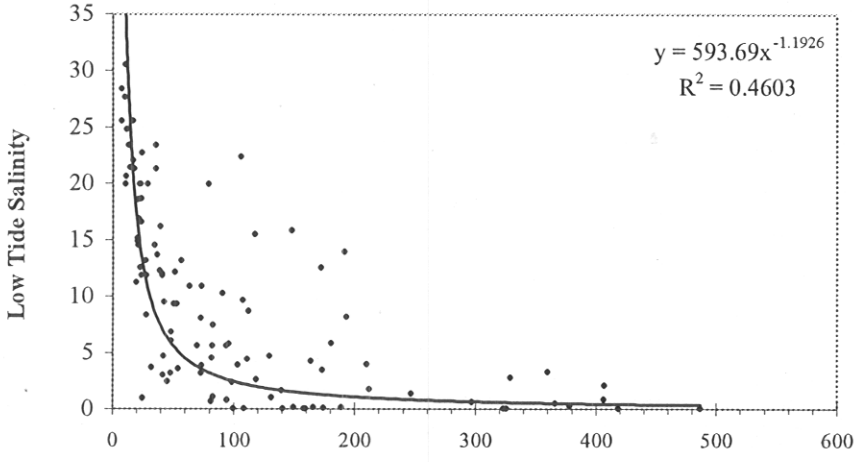

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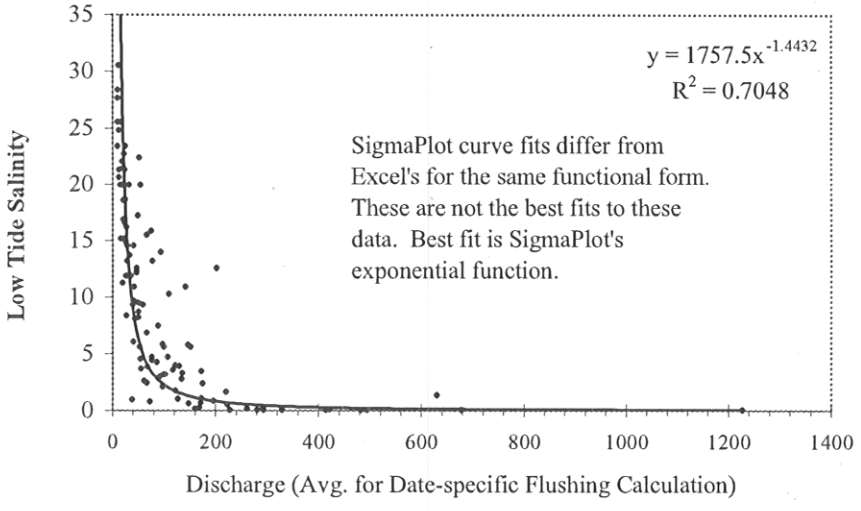
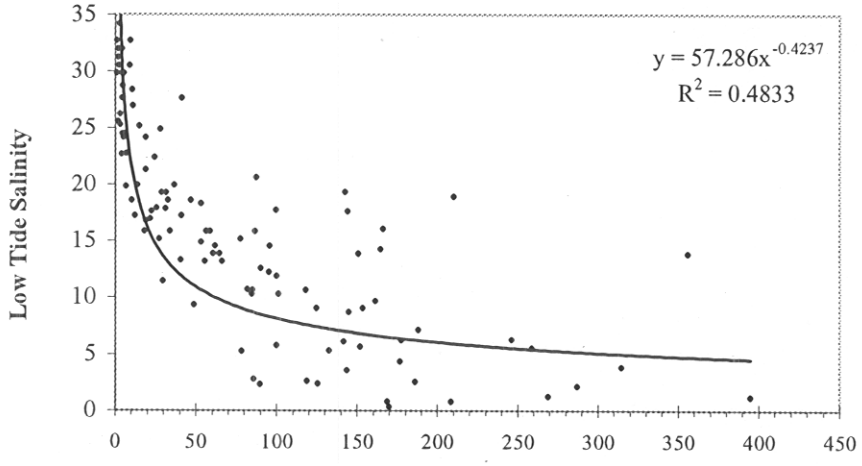


Satilla River, Buoy A15 on the Intracoastal Waterway
LMER 9.5 km



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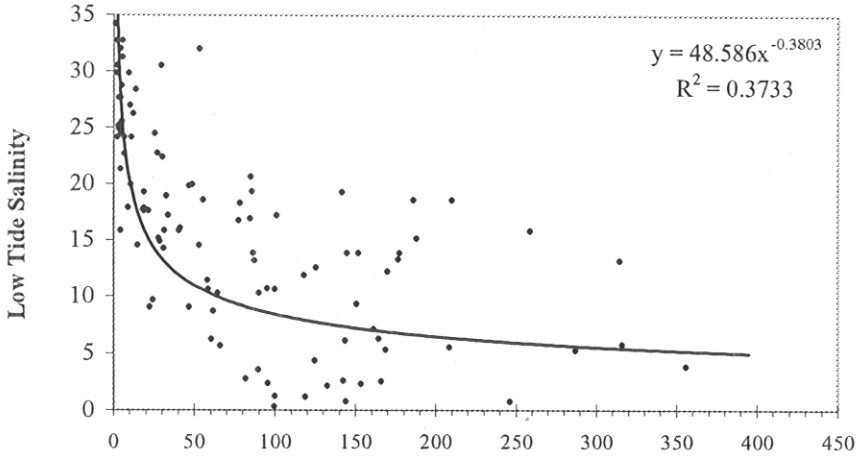

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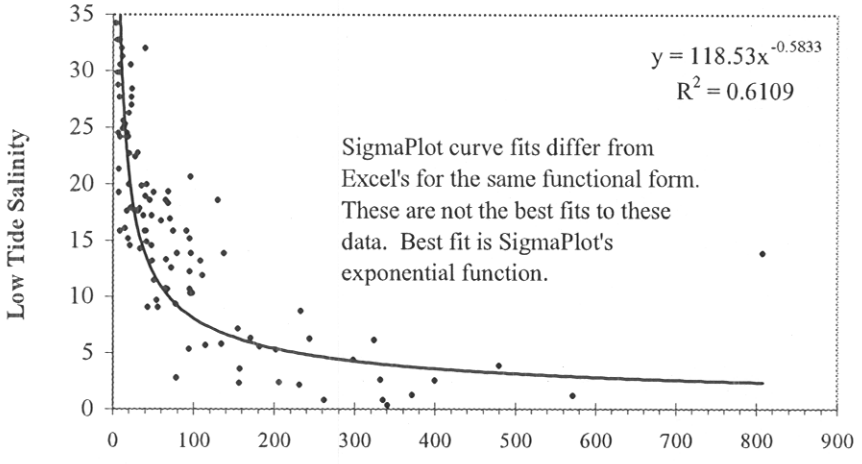

Summary and Recommendations:

I find that the District has adequately addressed most of my considerations with regard to the proposed MFL for the Loxahatchee River and Estuary. My final recommendations are summarized below:

1. Additional ground-truthing of the hydrodynamic model would be appropriate. This should involve both additional verification runs under current conditions as well as comparisons of historic salinity predictions with historic data, if available

2. It is critical to evaluate the data to see if salinity has in fact changed in the upstream portion of the river.

3. I suggest continuing the SAS analysis:
-relationships might be improved by re-evaluating which discharge to couple to with each observation
-consider using only the low flow portion of the data
-it would be useful to see graphs with the SAS fits and statistical analysis of the relationships

4. Consider rewording MFL to reflect the entire flow-discharge curve rather than focusing on one salinity/duration.